MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY.

COMMUNICATED BY E. B. TITCHENER.

XVI.—A STUDY OF CERTAIN METHODS OF DISTRACTING THE ATTENTION.

III. DISTRACTION BY MUSICAL SOUNDS; THE EFFECT OF PITCH UPON ATTENTION.

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The experiments described in this paper were made during the year 1896-'97. Their main object was to determine (1) whether there is any relation between the pitch of a musical note and its effect upon the attention, and (2) whether music, when used for purposes of distraction, has any direct dynamogenic effect, and, if so, what it is. We hoped also to get (3) some general results in regard to the value of the musical phrase as a distraction. A fourth question, quite foreign to our first intention, suggested itself early in the work; and the experiments were arranged so as to bear upon it as well as upon our chief problem. The attention of the subject was tested by his discrimination of lifted weights; and the side-issue which we tried to settle was (4) the question whether upward, downward, or double movements are best for the comparative estimation of lifted weights.

The method employed was that of right and wrong cases. The details of the experiment were as follows: The subject sat sidewise at a low table, with his right fore-arm resting upon it, the ulnar side down. His eyes were closed. Above his arm, and at right angles to it, was a small iron rod, to limit the distance through which the hand should be raised or lowered. The rod was wrapped with woolen cloth in order to prevent distraction from temperature sensations. Its height above the table was 22 cm. The weights used were two wooden boxes, like enlarged port-wine glasses broken off short in the stem and lidded, about 6.5 cm. in diameter at the top, containing fine shot. For two of the subjects (*Se.* and

P.) the weights were 250 *g.* and 265 *g.* respectively. For the third subject (*Sh.*) they were 250 *g.* and 260 *g.*[1]

At the word 'ready,' the subject put his hand in the proper position for the experiment, with the fingers bent in so that the thumb and fore-finger formed a ring. If downward movements were to be made, the hand was raised until the volar side of the wrist touched the rod, the elbow remaining on the table. Two seconds after the signal was given, the first weight was placed in the support formed by the thumb and fore-finger, and the required movement—upward, downward, or double—was made. As soon as the movement was finished, the weight was removed from the hand. Two seconds after the completion of the movement, the second weight was given, without a signal, and the movement was repeated.

The experiments were made in series of twenty-five each, and three different 'patterns' were used.[2] In the following list, *a* indicates that the lighter weight and *b* that the heavier was given first. Each pattern, whenever used, was repeated with the letters transposed.

First Pattern.	Second Pattern.	Third Pattern.
b	2 *a*	3 *b*
2 *a*	3 *b*	2 *a*
3 *b*	*a*	2 *b*
2 *a*	2 *b*	3 *a*
2 *b*	2 *a*	*b*
a	2 *b*	2 *a*
2 *b*	*a*	2 *b*
3 *a*	*b*	*a*
b	2 *a*	3 *b*
a	2 *b*	*a*
b	3 *a*	2 *b*
2 *a*	*b*	3 *a*
3 *b*	2 *a*	
a	*b*	
25	25	25

The distraction consisted of twenty-five phrases, of nine notes each, played upon the piano. All were written in $\frac{2}{4}$ time and in the key of C.[3] Five different octaves were used,

[1] We had assumed that a difference of 10 *g.* (*i. e.*, of $\frac{1}{25}$) would be supraliminal; but preliminary experiments showed that for *Se.* and *P.* a larger difference was necessary.

[2] *Phil. Stud.*, XI, 224.

[3] The following is No. 3 of the set of 'phrases,' some of which we adopted from G. Viehl's "Graded Studies in Sight Singing" (1896), while others we put together ourselves :

the notes ranging from the c^{-1} to the c^4. In each series of twenty-five experiments, five phrases were played in each octave. The order of the phrases was changed with each series, and no exercise was played twice in the same series. The succession of the octaves was so arranged that each occupied any given position (*e. g.*, the first) just as often as every other octave. Moreover, it was very seldom (only once in ten series) that any octave followed the one just above or below it on the piano.[1]

Experiments were made with three subjects: Miss S. E. Sharp (*Sh.*), Miss N. G. Seymour (*Se.*) and Dr. W. B. Pillsbury (*P.*). All were trained subjects. *P.* was 'unmusical'; *Sh.* averagely, and *Se.* exceptionally 'musical'. There were four classes of experiments:

I. Without distraction.
II. With distraction throughout the experiment.
III. With distraction in the first half.
IV. With distraction in the last half.

The four classes are referred to by Roman numerals throughout this paper.

In I, 600 experiments were made by *Se.* (200 for each of the three movements) and 450 by *Sh.* and *P.* In II, III and IV, *P.* performed 500, and *Se.* and *Sh.* 750 in each class. This gave *Se.* and *Sh.* 250 experiments for each of the three movements in each set, while *P.* had either 150 or 175 for each movement of each set.[2] Or, if we estimate according to octaves, in each class *P.* performed 100 experiments and *Se.* and *Sh.* 150 for each octave. The movement and the class of experiments were varied frequently, so as to insure equal degrees of practice in all.

The following Tables (A to D) give the results of our experiments so far as they have a bearing upon the question of *distraction*. In each of these tables, the double, upward and downward movements are averaged. The figures indicate the percentages of right judgments.[3]

[1] In order to secure this result and the still more important one of equal distribution of the octaves, it was necessary that the number of series taken should be some multiple of ten; *e. g.*, when double movements were used, each subject had to make either 250 experiments, or 500, or 750, and so on.

[2] This inequality is due to the fact that, as explained above, we could not use more than 500 experiments in any class, unless we had time to make as many as 750.

[3] These percentages are obtained in the usual way; $r^1 = r + \frac{e}{2}$. *P.* and *Se.* gave exceedingly few *e* judgments from the first. *Sh.* gave more; but the number rapidly decreased with practice. It does not seem necessary, for our present purpose, to print the full tale of *r*, *w* and *e*.

TABLE A.
(Class II.)

SUBJECT.	Octave I.	Octave II.	Octave III.	Octave IV.	Octave V.	Average.	No Distraction.
Se.	81	75	72	84	80	78	73
P.	75	81	72	80	73	76	74
Sh.	74	78	72	76	69	74	71

TABLE B.
(Class III.)

SUBJECT.	Octave I.	Octave II.	Octave III.	Octave IV.	Octave V.	Average.	No Distraction.
Se.	79	81	73	73	77	77	73
P.	84	75	74	84	79	79	74
Sh.	63	66	74	72	64	68	71

TABLE C.
(Class IV.)

SUBJECT.	Octave I.	Octave II.	Octave III.	Octave IV.	Octave V.	Average.	No Distraction.
Se.	85	85	81	79	79	82	73
P.	73	77	71	83	82	77	74
Sh.	76	67	76	74	69	72	71

TABLE D.
(Average of II, III and IV.)

SUBJECT.	Octave I.	Octave II.	Octave III.	Octave IV.	Octave V.	Average.	No Distraction.
Se.	82	80	75	79	79	79	73
P.	77	78	72	82	78	77	74
Sh.	71	70	74	74	67	71	71

In the light of these results we may consider the questions (1) whether music furnishes an adequate *distraction* and (2) whether the effect produced by it has any constant relation to *pitch*. Of our four Tables, A furnishes the safest basis for an answer to the first question. For it might well be that in II the music would have a different effect from that which it exercised in III and IV. If we give the music only in one-half of the experiment, we may be simply 'distracting' through that half, and not affecting the other half at all; or we may be altering the whole experiment. In the latter case two opposite results are possible. The music may facilitate the attention, by sharpening the contrast between the two parts of the experiment; or it may confuse the subject, and thus inhibit the attention. And in a case where the music facilitates in II, it may very well have the opposite effect in III and IV. Hence if the different Tables suggest different conclusions, we should give more weight to the testimony of A than to that of B and C, or even of D.

With *Se.* and *P.*, however, the conclusions drawn from all four Tables are the same; viz., that *in general the music facilitates* rather than inhibits the attention; though for *Se.* one case (octave) in fifteen, and for *P.* four in fifteen, show the opposite result. With *Sh.*, on the contrary, Table C shows a deviation from this rule which is sufficient to affect the average results (in D). If, however, we base our judgment upon the results of Table A, we see that for *Sh.* also, the general effect of the music is that of facilitation, though in one octave it slightly inhibits. We conclude, then, that for these three subjects music throughout the experiment facilitates the attention, and that music in one-half has the same effect five times in six; *i. e.*, in all the sets except *Sh.'s* III. Whether, in general, music in one-half helps *more* than music throughout the experiment, the figures do not show conclusively. With *P.*, the facilitation is greater in III and IV than in II; with *Se.*, it is greater in IV and less in III; while with *Sh.*, it is less in IV and gives place to inhibition in III.

We turn now to the second question. M. Féré concluded from the results of his experiments that the dynamogenic effect of a musical note is greatest in the middle octaves.[1] If

[1]Ch. Féré, "Sensation et Mouvement," Paris, 1897, esp. p. 35. Féré worked with single clangs, not with phrases; and warns his readers against confusing the direct dynamogenic effect of the note with the associative influence of the 'morceau'(p. 38). Our phrases, however, did not suggest a musical air, *i. e.*, were not associative. Hence the results of the two investigations are largely comparable. Again, Féré's curve (p. 35) appears to have been taken from a 'hypnotique.' But he distinctly says that the results obtained with

we grant this hypothesis for a moment, the question is at once suggested whether the effect upon attention follows the same law or the opposite one; *i. e.*, whether the middle octaves have the greater effect upon attention—either in the way of facilitation or of inhibition—or whether they have the smaller effect. There does not seem to be any reason for supposing *a priori* that the psychological effect should follow the same law as the physiological. And when we examine the Tables we cannot find conclusive testimony in favor of either alternative. *If there is any fixed relation*, however, it seems probable from these results that *the middle octaves have the greater effect upon attention.* If we leave *Sh.'s* results out of account for the present, we notice in those of *Se.* and *P.* (1) that in all but one of the eight cases (four for each subject) the middle octave shows the lowest percentages; and (2) that in four of the seven cases in which it is lowest its percentage falls below that of no-distraction, in two it is equal to that of no-distraction, and is higher in but one. Since there is so great a tendency for the percentages of the middle octave to fall below that of no-distraction, while yet the average results for the five octaves are in every case better than that for no-distraction, we cannot adopt the theory that the middle octave has the least effect upon consciousness. We must rather suppose that it has an effect so great that the point of maximal facilitation has been already passed and inhibition has set in. In some cases, the inhibition produces a sudden falling of the attention-curve below the line of no-distraction; *e. g.*, in *P.'s* results in A and D, we find the highest points represented in Octaves 2 and 4, while in Octave 3 the curve falls below the no-distraction line. This seems to show that the point of highest facilitation is immediately succeeded, not by a smaller degree of facilitation, but by actual inhibition. On the other hand, in *Se.'s* C and D, we have, after the maximal point in Octave 2, a lessening of facilitation which does not amount to inhibition. Of these eight cases, five (*i. e.*, all but *P.'s* A and C and *Se.'s* C) seem, when taken separately, to support the theory that the middle octaves have the greater effect upon consciousness: *e. g.*, in *P.'s* D, Octave 1 shows facilitation; Octave 2, slightly greater facilitation; Octave 3, inhibition; Octave 4, facilitation; and Octave 5, diminished facilitation. When we compare the five cases with one another, however, we find some discrepancies; *e. g.*, in *Se.'s* A

neuropathic and hysterical subjects differ only in degree from those gained with normal subjects (pp. 32, 33). Dr. Scripture's curve ("Thinking, Feeling, Doing," p. 86) is evidently a copy of Féré's, generalized and translated from *kg.* into *lbs.* *Cf.* the figures on pp. 87, 88 with Féré, pp. 37, 38, 43, 49.

and D the points of maximal facilitation seem to be in Octaves 1 and 4, while in her B they are in Octaves 2 and 5. We notice, also, two cases, viz., *P.'s* A and C, where one end of the attention-curve falls below the line of no-distraction. When we turn to *Sh.'s* results, we find even greater difficulties for the theory. Her figures in A exemplify it fairly well; but those of C and D are difficult to explain, while those of B directly contradict the theory. According to the results of B, the middle octaves affect consciousness least; for we have facilitation in Octaves 3 and 4, and inhibition in Octaves 1, 2 and 5. In the face of so much contradiction we must give up the attempt to deduce from our results any clear-cut relation between pitch and the effect upon the attention, although the indications are undoubtedly as stated above.

Table E shows the bearing of our results upon the question of *dynamogenesis*. As explained above, *a* indicates that the lighter and *b* that the heavier weight was given first. The first two rows of figures in each set (*e. g.*, III *a* and III *b*) give the percentages of right judgments for *a* and *b* respectively. In the rows marked *Diff.*, we have the amount by which the *a*-judgments are better than the *b*-judgments. In the rows marked Per cent., we have the percentage obtained by dividing this difference by the percentage of the *a*-judgments.

If the music has a dynamogenic effect, the fact should appear from an examination of the Table. Since there is a general tendency toward over-estimation of the second weight (time error), we should expect to find in I and II, as we do, a higher percentage of right judgments in *a* than in *b*. Now if the music, by bracing the muscles, makes the weights appear lighter, then in III, where the music is played only during the first half of the experiment, the difference between the *a* and the *b* judgments should be greater than in I and II. In IV, on the contrary, the music, which is played only in the second half, should counteract, either wholly or in part, the tendency to over-estimate the second weight. Consequently, the superiority of *a*-judgments over *b*-judgments should be either lessened or entirely destroyed—or, possibly, even changed to a superiority of *b* over *a*. Moreover, if M. Féré is right in maintaining that the dynamogenic effect is greater in the middle octaves, these results should be more apparent in Octave 3 than in Octaves 1 and 5.

Let us consider first whether there is any general dynamogenic effect. On the whole, it looks as if there were. For *P.*, the difference between *a* and *b* is, on the average, somewhat greater in III than in either I or II, though in some octaves this is not the case; while in IV it is, on

TABLE E.

SUBJECT.		Octave I.	Octave II.	Octave III.	Octave IV.	Octave V.	Aver-age.	No-Distract'n
Se.	II *a*	84	79	77	87	84	82	83
	II *b*	77	73	68	79	77	75	62
	Diff.	7	6	9	8	7	7	21
	Per cent.	8.3	7.6	11.7	9.3	8.3	8.5	25.3
	III *a*	86	84	77	80	84	82	83
	III *b*	73	78	69	65	70	71	62
	Diff.	13	6	8	15	14	11	21
	Per cent.	15.1	7.1	10.4	18.8	16.7	13.4	25.3
	IV *a*	93	90	82	83	85	87	83
	IV *b*	78	81	79	75	73	77	62
	Diff.	15	9	3	8	12	10	21
	Per cent.	16.1	10	3.7	9.6	14.1	11.4	25.3
P.	II *a*	90	86	73	83	76	82	81
	II *b*	60	77	71	75	68	70	68
	Diff.	30	9	2	8	8	12	13
	Per cent.	33.3	10.5	2.7	9.6	10.5	14.6	16
	III *a*	93	81	86	90	85	87	81
	III *b*	75	68	63	77	73	71	68
	Diff.	18	13	23	13	12	16	13
	Per cent.	19.4	16	26.7	14.4	14.1	18.4	16
	IV *a*	72	77	76	88	85	80	81
	IV *b*	74	77	66	77	79	75	68
	Diff.	2	0	10	11	6	5	13
	Per cent.	2.8	0	13.2	12.5	7.1	6.3	16
Sh.	II *a*	79	83	75	85	79	80	81
	II *b*	69	73	68	67	59	67	61
	Diff.	10	10	7	18	20	13	20
	Per cent.	12.7	12	9.3	21.2	25.3	16.3	24.7
	III *a*	76	79	87	81	78	80	81
	III *b*	50	54	63	62	50	56	61
	Diff.	26	25	24	19	28	24	20
	Per cent.	34.2	31.6	27.6	23.5	35.9	30	24.7
	IV *a*	75	69	78	80	67	74	81
	IV *b*	77	64	75	69	71	71	61
	Diff.	-2	5	3	11	-4	3	20
	Per cent.	-2.7	7.2	3.8	13.8	-6	4.1	24.7

the whole, much less than in I and II. With *Sh.* this result is even more marked. For every octave, we find the differ-

ence greater in III and less in IV than it is in I and II. In two octaves of IV, we even see the relation of *a* and *b* reversed: the *b*-judgments are somewhat better than the *a*-judgments. *Se.'s* results are less easy to explain. In III the difference is greater than in II, but less than in I; similarly, in IV it is less than in I, but somewhat greater than in II. We notice a fact, however, which may help to explain this discrepancy. For *Se.*, the difference between *a* and *b* is much less in II than it is in I. It is possible, then, that the general effect of the music may be to decrease the difference between *a* and *b* judgments. The reason for this is, perhaps, that the music, by acting as a facilitation, helps to keep the first weight in memory until the second is given. The first weight 'loses' less, in the interval between the two parts of the experiment, and thus the tendency toward over-estimation of the second weight is partly counterbalanced. If we grant this hypothesis, it may help us to reconcile *Se.'s* results with the theory that *P.'s* and *Sh.'s* suggest. In III, the working of the law of dynamogenesis would tend to make the difference between *a* and *b* greater than it is in I; but the facilitation in the first part of the experiment helps to counteract this effect; hence the difference is less than it is in I, though still somewhat greater than in II. In IV, since the music is played only in the second half of the experiment, it does not have this effect. Here we have nothing but dynamogenesis to counteract the tendency toward over-estimation of the second weight; and if we assume that the second factor is more important, we can understand why the difference between *a* and *b* in IV is so much larger than we had expected.

Table F may serve to bring this point out more clearly. The figures are taken from the last two columns of the rows marked Per cent. in Table E.

TABLE F.

CLASS.	Favoring *a*.	Favoring *b*.	Per cent. of Difference in Favor of *a*.		
			Se.	*P.*	*Sh.*
I	Time-error		25.3	16	24.7
II	Time-error	Facilitation	8.5	14.6	16.3
III	Time-error Dynamogenesis	Facilitation	13.4	18.4	30
IV	Time-error	Dynamogenesis	11.4	6.3	4.1

If one can draw any conclusion from these results, it is that in *Se.'s* case facilitation is a more important element than dynamogenesis in determining the relative accuracy of *a* and *b* judgments in III and IV; while for *Sh.* and *P.* dynamogenesis is the more prominent factor. We have no desire to assert that these results *prove* anything; but they seem to hint at the presence of a dynamogenic law, and also to indicate that a 'distraction' which acts as a facilitation will, if given with the first of two stimuli, partly counteract the tendency to underestimate it. That this counteraction is especially marked in *Se.'s* case may be due to the fact that she has had a very exceptional musical training.

We may now turn to the question whether or not the dynamogenic effect is shown most strongly *in the middle octaves.* If it is, the difference between *a* and *b* will be greater in III and less in IV for the middle octaves than for the others. For *Se.*, this relation is not found in III; while the fourth octave has the greatest difference, the second and third have the least. In IV, on the other hand, *Se.'s* percentage for the third octave seems to support Féré's theory; but the value of its testimony is lessened when we remember that, in order to explain *Se.'s* average results in IV, we had to assume that dynamogenesis has comparatively little effect with her. If

TABLE G.

SUBJECT.	MOVEMENT.	CLASS.				AVERAGE.
		I.	II.	III.	IV.	
Se.	Double	78	84	82	83	82
	Up	74	77	77	82	78
	Down	67	74	71	81	73
P.	Double	77	77	84	81	80
	Up	70	78	75	77	75
	Down	75	73	78	77	75
Sh.	Double	75	72	68	73	72
	Up	68	77	68	71	71
	Down	70	72	68	72	71

this is the case, it is difficult to understand the suddenness of the descent from Octaves 2 and 4 to Octave 3.[1] With *P.*, the difference in III is greatest for the third octave, as it should be; but unfortunately it is also greatest for this octave in IV. Moreover, the second octave in III presents a difficulty. With *Sh.*, in III, the third and fourth octaves show the least differences instead of the greatest; while in IV, the percentage of the third octave, though less than that of Octaves 2 and 4, is greater than that of Octaves 1 and 5. On the whole, then, *it does not seem that we can assert any essential relation between pitch and dynamogenic effect.*

Our last table, G, is designed to show which is the best movement for estimating lifted weights. The figures indicate percentages of right judgments. In getting the figures in the column marked *Average*, due account was taken of the fact that fewer experiments were made in I than in II, III or IV.

From these results it appears that *double movements* are uniformly best for *Se.*, are best for *P.* in three cases in four, and are, on the whole, slightly best for *Sh.* For *Se.* upward movements are better than downward, while for *P.* and *Sh.* the two movements are about equally good. If we look at promptitude and subjective certainty of judgment, we must put the double movement a little ahead of the upward, the upward very considerably ahead of the downward. Hence the curves of magnitude and delicacy of *SD* show the same general trend, but do not run strictly parallel (*cf.* Külpe, "Outlines," p. 50).

There were certain defects in our method of experimentation which, with more foresight, might have been avoided; and it may be well to mention them here. (1) Each series consisted of twenty-five experiments, or five in each octave. This gave in any particular octave a predominance of either *a's* or *b's*; and since several different 'patterns' were used, we found, when we came to average our results, that some octaves had more *a's* than *b's*, and others more *b's* than *a's*. Since *a*-judgments tend to be better than *b*-judgments, this introduced an error into the figures of Tables A–D. The amount of the error was computed, and it was found that in seven of the seventy-two cases in those Tables a correction of 1% was required. The correction is included in the figures as given. In no case does it alter the relations of any two octaves. The error might have been avoided if we had used

[1] It is interesting to notice that *Se.*'s IV is the sole case of the six that furnishes a good illustration for Féré's theory. Taken by itself, it is excellent evidence for the theory: it is only when we compare it with the other results of the same subject that it fails.

series of thirty experiments, and had arranged the patterns so that in each series the number of *a's* and *b's* in any octave would be equal. Our reason for not using a series of fifty experiments was that we found upon trial that the subject became fatigued before the series was finished. (2) The attempt to solve the problem of double, upward and downward movements in connection with our main question was probably unwise, since, as we have seen, it greatly limited the number of experiments that could be used. We were led to it by the consideration of its possible theoretical importance (*cf.* Miss Parrish's experiments, this JOURNAL, VIII, pp. 51, 52). (3) The order in which the phrases were played was varied, so that the subject might not expect any particular phrase at any time. No attempt was made, however, to arrange the succession in such a way that each phrase would occur in one octave as many times as in another. This would have been somewhat difficult, since the order of octaves was also changed with every series. Still, if we were to repeat the investigation, we should try to remove this possible source of error.

We may sum up the results of our investigation in the following points:

(1) In general, the effect of the music was to *facilitate attention*, both when it was played throughout the experiment and when it was played in one-half only. In other words, under the conditions in which we used it, the music did *not serve as a true distraction.*

(2) There is some evidence, though it is not conclusive, of a dynamogenic law operative in III and IV.

(3) There is no evidence of any constant relation between pitch and dynamogenic effect, and hardly any of such a relation between pitch and 'distracting' power.

(4) It seems probable that a slight distraction, given with the first of two stimuli that are to be compared, will counteract to some extent the tendency toward over-estimation of the second stimulus (time error).

(5) For the comparative estimation of lifted weights, double movements appear to be somewhat better than single (either upward or downward) movements.

POSTSCRIPT.

The three Studies, XII, XIV and XVI, were undertaken with the view of discovering a means of distraction that should be capable of gradation, uniform in its working and

applicable to normal subjects. With such a distraction it would be possible, on the qualitative side, to describe the attributes of mental processes given in the state of inattention, and, on the quantitative, to measure the magnitude and delicacy of sensitivity and sensible discrimination in the same state.

Unfortunately, the work done upon the question had to be divided up into three parts, each to be completed within a year. Although we foresaw that such work would necessarily prove incomplete and, so far, unsatisfactory, we still judged the attempt at a solution of the problem to be worth the making. Now that it is finished we have, at least, and apart from any positive results, done something toward clearing the ground for future investigators. As for the results themselves, they may be summarized as follows:

(1) The use of a stimulus as a distraction does not by any means guarantee distraction. The 'distracting' stimulus may, in reality, reinforce or facilitate the attention.

(2) Many stimulus complexes are inherently unsuited to act as distractors. Among them are addition and similar arithmetical exercises, spelling, translating, etc., and musical phrases.[1]

(3) It must not be assumed that the effect of a distraction is limited to that part of the experiment which it accompanies. The attention may be given to or abstracted from the experiment as a whole; and distraction during a part of the experiment may affect the attention to the whole. This fact very greatly complicates the distraction problem.

(4) The most promising distractors are those that appeal most directly to affection, the obverse of attention; the least promising are those that appeal solely to the 'intellect,' and themselves demand active attention.[2] Odors have proved capable of furnishing a satisfactory distraction series.

(5) When one goes behind the numerical results and enquires into the mental mechanism of distraction, one is at once introduced into the field of individual psychology. It is, therefore, probable that the requirement which we set ourselves to fulfill is, in its strict formulation, impossible of ful-

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[1]Some of these may, perhaps, be utilized in the future under the conditions laid down by Miss Hamlin: this JOURNAL, VIII, p. 62.

[2]So, too, Miss Hamlin: "When the addition was performed while there was any strong affective coloring of the subject's consciousness, it was usually a successful means of distraction." JOURNAL, VIII, p. 62; and Dr. Daniels: "The reading of interesting stories in a loud voice and with the greatest possible rapidity" proved a useful mode of distracting. Note that the loudness and the interest are both appeals to the passive attention, and that the rapidity secures some sort of continuity. JOURNAL, VI, p. 559.

filment. 'Uniformity of working' is, it would seem, not to be secured.[1]

In conclusion, we would remind the reader that these results are valid only under the conditions of our experiments. There are many fields, both of sense discrimination and of distraction, which we have not touched at all. It is quite possible that some of the presuppositions of our work are wrong; psychologists are not by any means at one in regard to the significance of the results obtained by the method of right and wrong cases, the meaning of the time error, etc. It is greatly to be hoped that in the near future the whole issue may be raised more thoroughly than we have been able to raise it.

E. B. T.

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[1]Hence I should avoid any such distractors as stories, musical airs, etc. They will distract, if they arouse associated ideas, and so possess an affective value; but the confusion of individual psychology becomes thereby worse confounded.